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BICHROMATIC AND CROSS CYLINDER TESTS*

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This paper will endeavor to explain static and dynamic bichromatic tests and compare them with cross cylinder tests.

Basically the bichromatic and cross cylinder tests are the same: that is, they both cause blur circles on the retina by making beams of light focus in front of and theoretically behind the retina. They are both made for the same purpose: that is, to determine the amount of spherical power needed by a pair of eyes under the conditions under which the test is made. The cross cylinder causes two foci of a single object by making half of the rays diverge with a minus cylinder and the other half converge with a plus cylinder. The bichrome test causes two foci of two objects, placed side by side, by absorbing all of the rays from one object except the green, and all of the rays from the other object except the red. Since green light is refracted more by the eye than red is, the focus of the green object will be in front of that of the red object. If the green focus is as far in front of the retina as the red focus is back of it, both images will appear equally blurred and the eye will be in focus for yellow or normal light.

I fixed my test chart up so that the bichrome test could be made with it by sticking red celluloid over half of one of the lines of letters and green celluloid over the other half of the letters of the same line, with adhesive tape. I wanted to use the thirty-foot letters because of the blurring, but had to use the twenty-foot ones because they were the only sized letters that I have two lines of on my chart. When the light is turned on behind this line of letters half of them are black with a green background and the other half black with a red background. If minus lens power is needed the red shows up the clearer and if plus lens power is needed the green shows up the clearer. Herein lies the superiority of this test over the ordinary subjective tests: that is, in the fact that you can tell when a hyperopia is undercorrected. In facultative hyperopia.

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that is, in hyperopia that the patient can overcome with ciliary action, the test letters appear as clear without the plus correction as they do with it in the usual type of subjective examining, because he accommodates the amount of his hyperopia. But with the bichrome test, even though the patient can overcome his error with his accommodation, he does not entirely do so, which makes the green part of the chart show up clearer than the red.

You are all familiar with the bichrome chart that has recently been introduced. Perhaps some of you do not understand why this chart uses red and blue instead of red and green. For the benefit of those of you to whom this may not be clear a diagram has been drawn to explain it.

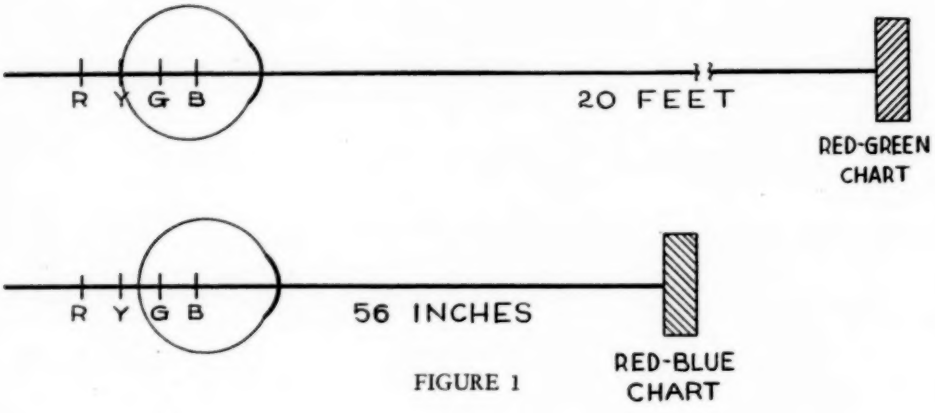

FIGURE 1

Fig. 1. In the top diagram the eye is looking at twenty feet at the red-green chart and the red focus is as far back of the retina as the green is in front of it, which makes the yellow focus (normal light) exactly on the retina. The blue focus is in front of the green. This is the static emmetropic eye.

In the bottom diagram the eye is looking at the red and blue chart, which is placed exactly fifty-six inches in front of the eye. Since the chart is placed closer to the eye than twenty feet, the focus of each color is carried back further than it would be if the chart were twenty feet away. The red focus has been carried far enough back of the retina and the blue focus back close enough to the retina to make them exactly the same distance from the retina, and their blur circles on the retina are exactly the same size, if the eye is focused at *infinity*, although *looking* at the chart fifty-six inches away. For the particular shades of red and blue used the manufacturers calculated that the chart would have to be placed exactly fifty-six inches in front of the eye to make the blur circles in a static

emmetropic eye exactly the same size, and consequently both images equally blurred. This explains why different colors must be used for different distances if a static test is to be made. If dynamic tests are to be made, as will be explained later, you only use the red and green colors that are used in the distance test.

An advantage that the red and green chart at twenty feet has over the red and blue chart at fifty-six inches is that convergence does not affect the results in the former, which allows you to check for binocular addition of plus power. If you let both eyes look at the red-blue chart at fifty-six inches they are, of course, converging to that distance, which affects accommodation. Even in the monocular test at fifty-six inches the eyes may converge some and affect the accommodation. Another advantage that the twenty-foot test has over the fifty-six inch one is that you are relieved of the trouble of placing the chart at exactly fifty-six inches from the patient.

The twenty-foot red-green bichrome is excellent for balancing the spherical power before the two eyes. Turn in three prism diopters base up before one eye and the same power base down before the other eye. Over-correct the plus or under-correct the minus before the eyes to make the red decidedly clearer. Reduce the plus power or add to the minus power over both eyes simultaneously and tell the patient to let you know when the colors are equally blurred in *either* eye. (Note: equally blurred, not equally clear, because both colors will never be clear at the same time.) When he reports that they are equally blurred in one eye, but not in the other, slowly reduce the plus power before the other eye until they are equally blurred in that eye also. This test will balance the spherical power needed by the two eyes to one-eighth diopter.

With the static bichrome chart you can work up or down, that is, you can build up from an under-correction or down from an over-correction. It is a good idea to go through both procedures in each case. When they both indicate the need of the same spherical power, as my experience indicates they usually will, you may feel satisfied that you have found the spherical correction needed.

Now let us consider the cross cylinder. I use it in every case, but not to determine the near accommodative need, because I think I have found a way to do this more satisfactorily with a dynamic bichrome test. Before explaining this perhaps I had better explain just how I use the cross cylinder.

I use it to find the amount of astigmia present. For this purpose I have a quarter diopter cross cylinder on a handle. If objective methods do not reveal any astigmia I am not satisfied that a small amount may not be present until I have checked the whole semi-circle at 30 degree intervals with my cross cylinder. I turn into my phoropter a quarter of a diopter of minus cylinder axis 180. Then I hold up my cross cylinder with the plus axis over the minus axis of the cylinder in the phoropter and then reverse it to the minus axis over the axis of the cylinder in the

phoropter and ask the patient which way the letters are clearer on the distance chart if there is any difference. If there is less than a quarter of a diopter of astigmia at this axis he will report that the letters are clearer when the plus axis of the cross cylinder is over the minus axis of the cylinder in the phoropter. If this proves to be the case I rotate the axis of the cylinder around the semi-circle at 30 degree intervals. I am satisfied that there is not as much as a quarter of a diopter of astigmia present if this proves to be the case at each of the 30 degree intervals.

When I first started using this technique I was surprised at the frequency with which I would find a small amount of astigmia, usually with oblique axis, that I had formerly been overlooking. It does not take long to go through this procedure. In fact, you can do so as quickly as I have described it to you.

Now let us consider the bichrome test at the near point, or what we might well call the dynamic bichrome test. I believe that this can be used instead of the usual cross cylinder test more easily and with better results because of the faults of the cross cylinder test.

A decided weakness of the cross cylinder test is that you must be sure that astigmia is accurately corrected before you can put any dependence on it. How often are we absolutely sure that such is the case? You will often find a discrepancy of a quarter of a diopter or more between the subjective and objective astigmia. There is often a difference between the static skaimetric and dynamic skiametric astigmia. That being the case, why should we not suspect a difference between static subjective and dynamic subjective astigmia? If you will look for this you will often find it. I have a patient who has astigmia with the rule at a distance, both subjectively and objectively, and astigmia against the rule are near, both subjectively and objectively. As a rule we do not bother to measure the dynamic subjective astigmia, so we are not sure that astigmia is not affecting our cross cylinder findings.

Another weakness of the cross cylinder test is the difficulty of holding the cross cylinders before the eyes while lens changes are being made. If you use a pair of cross cylinders on a handle you need three hands to hold it up and make simultaneous binocular lens changes. The phoropter has a pair of auxiliary cross cylinders to be inserted in the lens holder, but if the cylinder which corrects the astigmia is not exactly horizontal or vertical, you can not always place axis of the cross cylinders the same way: that is, sometimes you will have to place the minus axis horizontal and sometimes the plus horizontal, which adds to the confusion of the test. Then, too, the patient says sometimes that he sees colors around the lines. This happens too often to be purely imaginative.

On account of the above weaknesses of the cross cylinder test I began to experiment with a near bichrome test to take its place. I first tried red and green inks, but could not make the test sensitive enough in this way. It then occurred to me to use the same celluloid that is used in the distance test.

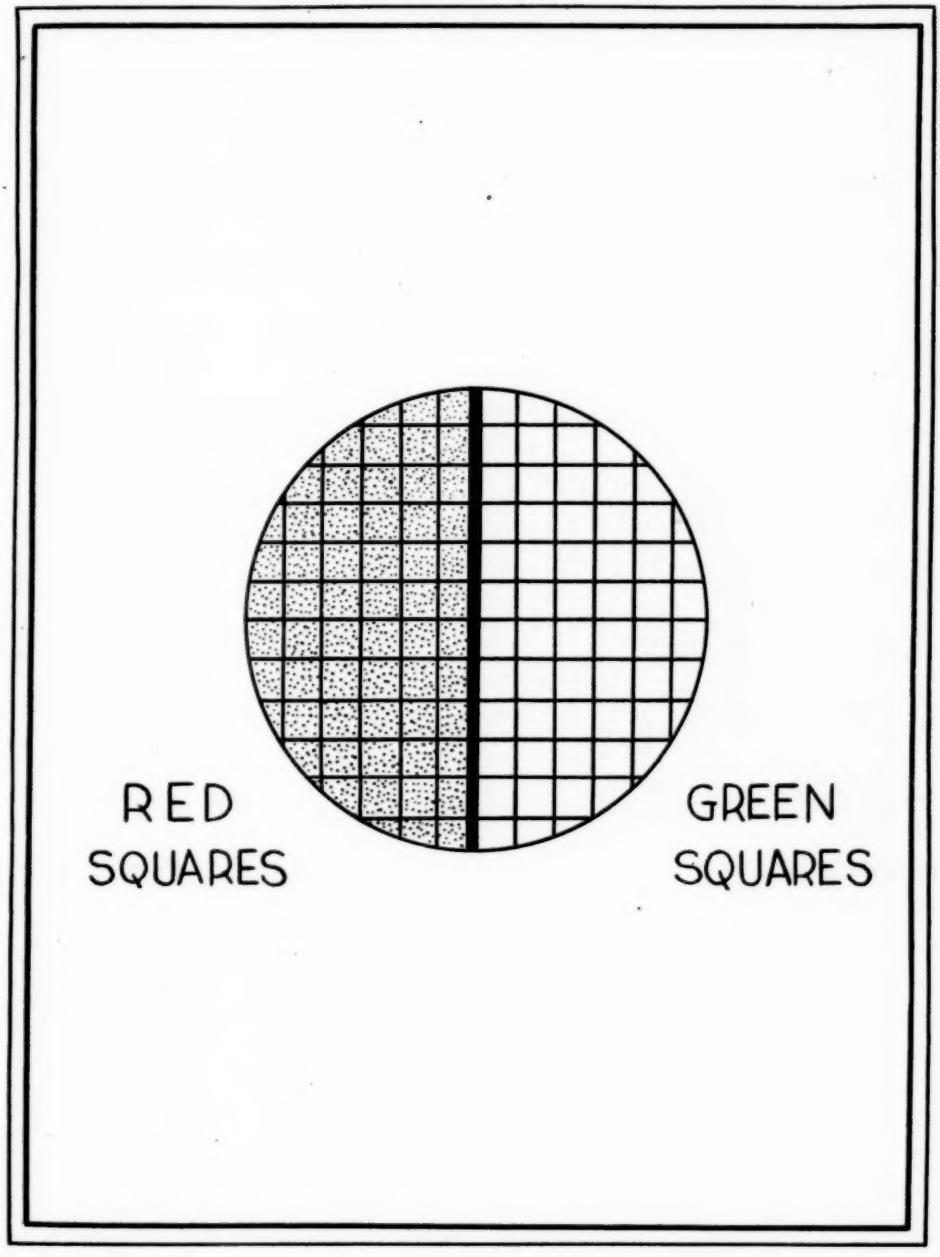

FIGURE 2

I made up a chart by using a card with a round hole in it 24 millimeters in diameter. Over the back of this hole, I stuck with adhesive tape, some of the red and green celluloid with inked cross lines on a piece of paper back of the celluloid. That is all there is to the chart. From the front the patient sees the red and green squares through the hole in the card. If he needs plus power the green squares are the clearer and if he needs minus power the red squares are the clearer. If the red and green squares are equally blurred his accommodation and convergence are in balance for the distance at which the test is made.

The findings with this test have been checked with the cross cylinder findings on the same patient, at the same distance, at the same time, and they agreed, which is what you would expect, because the optical principles of the two tests are the same. Both tests are dependent upon blur circles on the retina caused by two foci, one in front of and the other behind the retina.

This test seems to be particularly good in presbyopia to determine the addition. Just place the card at the near working point and add plus power until the green squares are no clearer than the red squares. You can not find the amplitude of accommodation with it, because when you move the card closer than the point at which you have balanced the accommodation and convergence the green squares will get clearer. You will have to find the amplitude in the usual way.

In pre-presbyopes the findings with the bichrome card are not as definite as in presbyopes for the same reason that dynamic findings often fluctuate while you are scoping young patients. The same thing is true of the cross cylinder test. There is a certain amount of play between accommodation and convergence in young people. Of course the bichrome test does not cause this play to be any less active than it is in dynamic skiametry and the cross cylinder test, but on the other hand this play is not any more active in the bichrome than in these other two tests. Therefore, it would seem that the findings are at least as dependable with the bichrome card as with any other dynamic tests, and probably more so, because, as explained above you do not have astigmia to contend with as you do in the cross cylinder, and doubtful convergence to contend with as you do in dynamic skiametry, on account of the blinding effect of the light in one eye.

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SOME RECENT DISCOVERIES IN ANATOMY, PHYSIOLOGY AND PSYCHOLOGY OF IMPORTANCE TO OPTOMETRISTS*

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In every field of human activity, modern experiment and study have brought to light many previously unknown facts about nature that have modified and changed opinions, customs, laws. Wherever men have taken advantage of these findings, there has been a noticeable improvement in the products of their manufacture, in the ways of their living, in the methods with which the various professions and occupations attack and meet their problems.

But wherever there is a group that stubbornly oppose themselves to the changes indicated as advisable by these discoveries, we see a group that stands still. If it does not actually go backward, it seems to have retrogressed when compared with the advances made by other groups of more alert and less conservative mind.

During the latter half of the nineteenth century appeared a small number of experimental investigators in physiological optics. These few names we hold in merited veneration. Unfortunately, these were followed by a much larger group of purely speculative writers who endowed us with a vast amount of hypothetical nothings that we have had the misfortune to have accepted seriously. So strong has been their influence that we still find ourselves in their thrall, speculatists still thrive and their hypotheses grow ever more burdensome, clogging our progress.

The art and practice of refraction is the last of the professions to emerge from the mists of traditional philosophy. We are therefore under this handicap, that our current literature, instead of being a library of facts gained by research and actual experiment, is largely a re-hashing of the speculations of the period from 1880 to 1920, when there was little research work done but during which period there was rife what seems to have been a contest of guessing, each writer trying

*An abridgement of the material presented before the American Academy of Optometry at Omaha, December 15, 1930.

to outdo all others in the wildness and vagueness of his postulations. But at the time, many of these fallacious theories were accepted as truths, no one asking for substantiation by carefully controlled experiment.

Practically all our theories of ocular functions were written in the days of the old philosophical psychology, when savants solved problems of human actions and behavior by thinking about them, instead of experimenting with them. Also, in those times knowledge of the anatomy and physiology of the body was extremely meager. Means for investigation were few and crude. So many of the theories were based on insufficient data, but with the unfortunate assumption that the data formed adequate basis for the conclusions drawn.

Now we are entering a more fortunate era, when as in other fields, we are demanding proved facts. We are learning to be skeptical of these philosophical traditions. But here we are bound to encounter skepticism of the new by those who would adhere to the old. And then we come to the conflict between the experimentalist and the theorist, which has waged throughout man's known history. The experimentalist condemns the theorist, is impatient of his stubborn clinging to the past. The theorist fears the experimental worker, who would tear away the footing, insubstantial though it is, on which he has rested. And since he is so fearful, his condemnation is violent. The experimental worker is treading new paths, wandering in unknown fields, departing from the beaten track. The theorist bases his concepts on tradition, questions not its authenticity, provided it shall have the prestige of age and a famous name. He will accept only such experimental data as will justify or assimilate with his theories. He rejects any physical data as being superfluous if their incorporation or consideration with his previously accepted ideas should endanger his hypotheses.

The theorist's shibboleth is "authority." The empiric worker flouts all authority, ignores precedent, relies on experiment alone and cares not if his findings agree with past views or open new vistas. He draws his conclusions with hesitation and doubt, fearful lest he may have missed some fact and so have come to wrong conclusion. He resorts to agreements for understanding only when direct experimentation is impossible and withholds his final assent or dissent until improved tools and technic shall bring direct experimentation within reach. He formulates postulates from his data that he may have something to use as pick and shovel for deeper exploration. He is ready to throw all theories, all hypotheses, all postulates, overboard at any time, if some newly uncovered fact casts the slightest shadow of doubt upon them. His thought of yesterday cannot be his thought of today, since today he has more facts that will modify his version of what he had found before. Tomorrow he will have more facts, and will again revise his working postulate. He who is studying nature must always be changing his mind, for nature's laws are hard to discover, a lifetime of patient delving can only bring to light a very little.

But the theorist is always dogmatic, always sure he has all the knowledge obtainable or relevant, instantly rejects as unimportant any observation that conflicts with his pre-suppositions. He is disdainful of the plodding experimental, or empiric, worker, who wanders hither and thither, picking up little items of physical data, storing them away, classifying and tabulating, rejecting nothing as irrelevant. The theorist has even gone so far as to change the meaning of that noble word, empiric, which means experimental, and dubs the empiricist, who believes in experiment only, "a quack." But it is the empiricist, the iconoclast, who has led the world forward. Ever, it is the war between the fundamentalist with his traditions and mysticisms and the modernist with his experimental research and love for truth.

Many of the writers of texts on ophthalmological and optometric subjects remind us of that famous traveler of the early 17th century, Sir John Mandeville, who wrote copiously of countries he had never visited. We can smile, now, with a little tolerance, at many of these speculatists, even though we are vexed with their misleading papers and books, with the methods and techniques they advocated which are responsible for the inefficiency of many refractionists and the discomforts of many scores of people wearing misfitted glasses. But we cannot understand nor view with anything but disgust the editors of standard anatomies and physiologies who set down as facts derived from actual dissection or experiment certain items of intimate anatomy about which actual workers have shown nothing whatever is known. It had been far wiser to have written "nothing known" than to have led us so far astray.

It is out of the question to review in this short paper all the many interesting items that modern research in physiology, psychology and anatomy have brought to light that pertain to our practice. I can only mention a few to show how much we have known that was never true, how little we actually do know, how necessary that we finally abandon much of our theory, how important that we devote ourselves wholeheartedly to the matter of serious research, intrusted to fearless, capable, properly trained minds that are untrammeled by preconceptions, free from blighting influence of dead *soi-disant* authorities.

Accommodation of the Crystalline Lens

The behavior of the crystalline lens when being accommodated for the purpose of focusing is as much a mystery as ever. The work of Fincham (1922-26), A. Vogt (1924), J. B. Story (1925), confirm and considerably augment the original observations of Tscherning. Fincham showed that there is considerable variation among individuals as to the convexity of the anterior surface in the act of accommodation. It seems definitely accepted now that the anterior surface bulges forward in the act of accommodation, that it assumes something of an hyperbolic curvature, the center bulging forward and the peripheries flattening, that antero-posterior diameter increases, that the equatorial diameter decreases. (References: Transactions of the British Optical Society, 1924, page 224; Proceedings of the Optical Convention in

London, 1926, page 454; Transactions of the Ophthalmological Society of the United Kingdom, 1924, page 413.) Fincham's work was so carefully done that no one doubts the authenticity of his reports.

But how or why this change takes place, no one knows. There has been much study of the action of the suspensory ligaments and the lens capsule, but the results have been negative so far as definite information goes, although there have, of course, been a number of contradictory theories advanced. It seems evident from the various reports of experiments that the capsule is held in tension when accommodation is at rest, is relaxed when accommodation is exerted. But the evidence is not wholly conclusive. We hardly find it necessary to go into these reports because of their indefinite results.

We might spend a moment on the studies of Luedde. (American Journal of Ophthalmology, 1927, page 15). He makes some interesting remarks, rather speculative, on the possible antagonistic action of the radial and meridional fibers of the suspensory ligaments. But he observed with the slit lamp, in some cases of subluxation of the crystalline, some action that might suggest that perhaps the vitreous is pushed forward during accommodation. When we remember some odd case reports and observations made by various clinical workers and the suggestion that pops up every little while that perhaps the anteroposterior distance from fovea to cornea is shortened during accommodation, it is very apparent that there is something about accommodation that no one has yet learned.

Among postulates that have been advanced in the past is one that possibly the ligaments of the zonule of Zinn exert a sort of muscular action. The dissections of Casavieja (reports not yet published), using methods and stains developed by Cajal, show that this cannot be true.

There has been nothing learned to show that the guesses of either Helmholtz or Tscherning can be substantiated.

The Ciliary Muscles

We have been taught to look at the ciliary muscle as a tripartite, consisting of the meridional fibers, the radial fibers, the circular fibers. Little was known as to the functions of each. The meridional fibers are said to be hypertrophied in known myopic eyes. This has seemed to be the case in a number of prepared sections of the ciliary muscle of known myopic eyes that I have personally examined. There does not seem to be much variation in the radial fibers in different types of eyes so far as anyone has left a written record nor so far as I have been able to see during my own microscopic studies. The circular fibers are always hard to find, are totally absent in many dissections, or in others are very few and small. In an occasional slide one can find them quite well developed. It has been written in the anatomies that they are hypertrophied in hyperopic eyes. The origin of this statement I have never been able to find, nor do any of the writers state they have verified it.

From the mechanical study of their arrangement, it has been assumed that the circular and meridional fibers both act in a positive way to draw the ciliary processes forward and thus to relax tension on the ligaments of Zinn, permitting the increases in convexity of the crystalline that have been noted in the act of positive accommodation. The radial fibers, it was believed, tighten the ligaments, by drawing the edges of the ciliary processes back, thus exerting a tension on the capsule and flattening the crystalline. Thus there were conceived two sets of muscle fibers that acted in positive accommodation with one set acting antagonistically for negative accommodation.

Here we may note that the observations of Luedde on the ligaments of the zonule of Zinn, if they are in any part true, would tend to the discredit of these theories.

Today we find a number of anatomists, working with improved microscopes, better dissecting methods so that they can make finer slices and thus see structures more intimately, with better staining methods, with improved technique in all directions, who seriously doubt the existence of the circular fibers as a separate muscle. Rather, they incline to the thought that perhaps the circular fibers are really tetanized bunches in the radial fibers.

This throws doubt on all theories of accommodation, leaves us with no basis on which to theorize further, teaches the folly of speculation without further data. In time, research methods will be so improved that the facts concerning the anatomy and functionation of the ciliary muscles will be known.

Nothing is definitely known about the innervation of the ciliary muscles. There is traced a para-sympathetic supply. Whether or not there is a spinal sympathetic supply has not been learned. Many notable and competent anatomists have spent much time searching for traces of spinal sympathetic innervations of the ciliary muscles, but so far have been unable to find such. That, however, is no proof of its absence.

It has been stated positively for many years that the ciliary ganglia are purely para-sympathetic. This is now in doubt. Finer dissections have shown other than para-sympathetic cells in these ganglia, but these strangers have not been identified. It is evident that the ciliary ganglia of different species of animals differ widely, so there is no help from comparative anatomy.

The question of spinal sympathetic innervations of the ciliary muscles is probably the most important anatomical problem that optometry needs solved. On it hinges the question of whether or not the use of atropine or other cyclopegic affords a reliable method for determining the refractive error of an eye. When this is known, we shall either have a valid scientific foundation for opposing it, in place of the somewhat sentimental reasons now advanced, or we shall have no such definite reason, from that source, at least. As scientific men, seeking truth only without regard to what its findings, we wish most

earnestly to see this problem solved. Some of us had hoped that the Academy would select this as a research problem, placing the work in competent hands. It would perhaps require many years and the guarantee of a very considerable amount of money. But no problem of such practical significance could be chosen. For any work along any line must remain uncertain until this one question is answered. Any result obtained in any research must remain dubious until this one foundation fact is ascertained.

It can be shown by direct experiment that the sphincters of the pupils are innervated via para-sympathetic routes, that the dilators of the pupils are innervated by the spinal-sympathetics. Para-sympathetic termini have been found in the sphincters. Spinal sympathetic termini have not been found in the dilators, but their presence can be proved by watching the pupils when stimuli is applied or taken away from the nerves, or by removing their ganglionic roots. Para-sympathetic termini have been found in the ciliary muscles. Spinal sympathetic termini have not been found in the ciliaries any more than they have been actually seen in the dilators of the pupils. But direct observation is not so easy when it comes to studying the action of the ciliaries.

Now comes some new information that may or may not prove to have some bearing on this problem. Look in your anatomies and you will see the dilator muscle of the iris pictured as lying beneath the plates and ending at the base of the iris, with no visible point of attachment. Berner, in 1926, found that these pictures are not true representations. The earlier anatomists lacked the instruments for dissection that we have today, so their work was necessarily rather coarse. Berner found that the dilator of the iris has fine muscle cells extending down into the ciliary body and prolonged in places to find attachment to the pectinate ligament. So the roots of the meridional and radial fibers of the ciliary and the roots of the dilator of the iris are nearer to apposition than we had supposed. (British Journal of Opthalmology, 1926 and 1927.)

Remembering that the meridional fibers of the ciliary are hypertrophied in at least some myopic eyes and that one of the first signs of the onset of myopia is dilatation of the pupils, we can wonder if there is more than mere coincidence in the anatomical juxtaposition of the attachments of the dilator and the meridional muscle, if in time they may not be found with a common spinal sympathetic nervous supply. But we may do no more than wonder, we dare not assume much without more data.

One more very valuable item. Pearcy and Allen, in 1927, found that accommodation is reduced from 2 to 5 diopters by internal pressure in the stomach or the lower part of the colon. This was found by inserting small balloons into these organs. They also found that the veins of the retina became enlarged. Perhaps this explains some anomalies of accommodation and the poor vision we frequently meet with some of our middle aged patients who complain of "gas" or "wind" in the stomach and in some cases of chronic constipation. This

is worth further study and now that the fact is established by these experiments, clinical investigation is authorized. (Reference: American Journal of Physiology, 1927, page 56.)

The more research is done, the more evident becomes the truth of Gaskell's remark in 1916, "The ciliary muscle is different from any other muscle in the body."

The same may be said about the short ciliary fibers. They are of the para-sympathetic nervous system, but their histology is that of voluntary nerves.

The functional difference betweeen the involuntary system and the voluntary system is that in the voluntary the motor nerve serving the muscle arises from a nucleus in the central nervous system, while in the involuntary, or autonomic, nervous system there is a relay system of pre-ganglionic fibers from the nucleus in the central system to a ganglion, or aggregation of nuclei, nearer the muscle, and the post-ganglionic fiber from this outside nucleus into the muscle.

The motor nerves from the third nucleus to the ciliary muscles function according to the involuntary system rule, there being the pre-ganglionic fibers to the ciliary ganglia, the post-ganglionic fibers from these ganglia to the muscles. But in appearance and structure, these fibers more nearly resemble voluntary system nerve fibers than they do the other fibers of the para-sympathetic system distributed through the body.

In time, this anatomical fact may prove to have a bearing on the physiology and psychology of the function of focusing. It is hard for many to believe that accommodation, even without convergence association, is a purely involuntary system function.

Pupillary Contraction to Light

Our anatomies describe in great circumstance and detail the sensory and motor pathways of the light reflex, from the retina back to mid-brain and out to the sphincter of the iris. Now it appears that this was entirely hypothetical and the only thing known is that these descriptions are not true at all.

The afferent fibers carrying these excessive light sensations that would reflexly close the pupils have been traced as far back as the posterior parts of the optic tracts. There they disappear. The motor fibers have been traced from the Edinger-Westphal nuclei to the ciliary ganglia, thence to the sphincter muscles. How the impulse travels, where it is converted into the motor reflex, how it gets there, is unknown.

I quote this merely to illustrate how much of what we thought we knew we find we do not know at all.

An oddity of the sphincter has been demonstrated. Hill and Azuma demonstrated that the sphincter will react to ultra-violet directly, without the sensation having traveled back to the brain, that is, without the necessity of a nervous system. This is a quality of primitive muscle, for in primitive animals without a nervous system muscle

reacts directly to contact stimulus. (Loeb: Comparative Physiology of the Brain.) The sphincter of the pupil seems to have partially retained this primitive quality. Some other investigators have learned that the sphincter will react directly, without nerve system intervention, to direct sunlight. (References: Recent Advances in Physiology by W. S. Duke-Elder; Hill and Azuma, in Transactions of the Royal Society of London, Vol. 99 B.) Hertel's experiments were particularly interesting but unfortunately have not yet been made available in English. He severed all sensory and motor connections from retina and sphincter and found that the pupil would contract to bright sunlight.

The Fourth Nerve

We are taught that the fourth nerve is entirely decussating, that all fibers from the right fourth nucleus pass to the left superior oblique, that all fibers from the left fourth nucleus pass to the right superior oblique. However, Kidd states that this is not always true. It seems that originally these nerves were uncrossed and that the decussation is still in the course of evolution. This may have a bearing on some binocular difficulties we encounter, as peculiar cyclophorias and hyperphorias. Still, it is pretty hard to know how to make practical use of this information. (Reference: British Journal of Ophthalmology, 1922, page 49.)

The Superior Oblique

There are three theories as to the direction in which the superior obliques turn the eyeballs. First, the old anatomical theory, downward and outward. Second, the theory held by a large group of neurologists, upward and inward. Third, the theory held by most modern physiologists, inward and downward. Present day thought seems to tend toward the idea that the superior obliques are the chief muscles of convergence in the associated bi-foveal fixation and focusing, or convergence-accommodation, act. All clinical evidence supports the latter view. As to the old anatomical idea, which some one here is bound to bring up, that because of the peculiar passage of the muscle through the trochlea and because of its insertion in the sclera, its mechanical arrangement indicates that it can turn the eyeball in one direction only, viz., down and out, let us remember that a great many similar notions of the early anatomists concerning the mechanical action of muscles as derived from their configuration have been found to be untrue.

I have studied the insertions of the superior obliques in twenty enucleated eyes and have not seen any two alike. In the series of stereoscopic photos of the external eye prepared by Elsching and Wooton, much stress is laid on the variations in attachments of the muscles, particularly of the superior obliques, in the eyes photographed. Studying these insertions, one might say that from the anatomical configuration each of these eyes would perhaps turn in a different direction by contraction of their superior obliques.

According to Kidd, whose paper I have just quoted, the fourth cranial nerves and the superior obliques are the last of the ocular neuromuscular system to have been segregated. Perhaps this is why they are

subject to such considerable variation.

Tilney and Riley, in Form and Functions of the Nervous System, state definitely, without the slightest hesitation or modification, that the superior obliques are the chief muscles of convergence. This work is remarkable for the very little uninvestigated reprinting of antique suppositions. Hence, one is inclined to credit the statements of its authors as based on some actual work rather than on mere hypotheses.

I might add here that my son and myself have discovered a new method of studying movements of the eyes by still photography, in addition to the studies we are making by motion photography. We reflect a spot of light on the corneas and make a time exposure, of one-half second, one second, two or five seconds or longer, as the case may be, while the eyes are in motion. On developing the plate, we find that the spot of light has traced a line, or rather, that the two spots of light have traced two lines, one for each eye, showing exactly the movements of the eyes during the period of exposure. This can be greatly enlarged and studied, even to dividing the parts of the lines into the flights and perches in 1/100 seconds. The advantage of this method is that the oblique motions are shown very definitely. At the next meeting of the Academy, we hope to be able to show some definite information from these studies. Oblique motions are not caught in motion picture studies. The pictures we have to date, show very decisively the influences of the obliques in convergence. In one picture, there were over a hundred oblique motions in an exposure of 1/10th of a second.

The Third Nucleus and the Third Nerve

In our text books on ophthalmological and optometrical practice, which unfortunately do not go into detail on many very important subjects, the anatomical aggregation of motor nuclei known as the third cranial nucleus is presented as a functionally single nucleus, the bundle known as the third nerve, or oculo-motor nerve, as a functionally unified nerve. We are taught that all parts are in association and that all muscles fed through this great trunk system are innervated in association. Nothing could be farther from the truth nor has there ever appeared any anatomical or neurological data that would afford a basis for such an idea.

Ophthalmologists and optometrists seem to have assumed this, without giving any attention whatever to the work of the anatomist. Very little that is definite is known about this nucleus and nerve. It is made up of a number of parts, some seeming to be connected by association fibers and some seeming to be entirely independent. There is also a great deal of contrary opinion concerning its associations with other cranial nuclei, some anatomists thinking that collaterals from the seventh and eighth nuclei reach to the third and are in direct control of its functions, while others seem to think that these collaterals reach as far as the sixth only, there synapsing and passing on to the third and fourth nuclei their integrative impulses. All such associations, which are undoubtedly in effect, are still hypothetical so far as anatomical

pathways have been demonstrated.

The central portion of the third nucleus is demonstrated in a satisfactory way to be an outlying ganglion of the para-sympathetic system. Recent anatomists are giving great attention to the Edinger-Westphal sections, which seem to be the chief nuclei through which control of positive acting accommodative muscles and the sphincters of the irides are mediated.

Along each side of these mesial portions of the involuntary system, is a string of irregularly arranged voluntary nuclei, which seem to be integrated by connector fibers but which have never been demonstrated to have any connection with the involuntary mesial portions. It has been proved that these voluntary lateral nuclei control the extrinsic muscles of the eyeballs, excepting the superior obliques and the external recti.

Semi-decussation of the motor fibers from these voluntary nuclei, so that the muscles of both eyes are integrated in their activities through the voluntary parts of the third nucleus has been determined by anatomical dissection. Anatomists have always agreed that the decussating fibers emerge from the caudal part of the nucleus. That is about the only point on which there is agreement.

A very valuable addition to the knowledge of the third nucleus and the third nerves is given by Perlia, who claims to have proved that the caudal portion of the nucleus is a separate functional unit, with semi-decussating fibers, in control of the convergent act. This, of course, will need further confirmation. This lower medial section was named "Perlia's Nucleus" some fifteen years ago, but no mention of it has entered text books on refraction that have appeared since. We might add that Perlia's work has been confirmed by Kappers, Brouer, Mann, Paton. The earliest appearance of Perlia's nucleus is, I believe, found in rudimentary form in some members of the cat family.

Whatever association there is between involuntary system control of the intrinsics and voluntary system control of the extrinsics must occur at some point behind the third nucleus. It cannot occur in the nucleus itself. There is no evidence, either anatomical, neurological or clinical, that the assumption of direct association of the internal recti with the ciliaries has the slightest foundation. Neurological data points more toward functional association with the ciliaries of the superior obliques from some point in the mid-brain, probably within the pregemina.

The whole matter of bi-foveal fixation and focusing is one of functional integration of several parts which also retain, to a greater or less extent in various individuals, some degree of independence for functional integration in other motions than convergence and accommodation. But this integration does not occur in the motor nuclei directly supplying the muscles but at some point or points farther back in the pathways.

Instead of talking so much about associations of the internal recti

with the accommodation of the crystalline lens, or of their associations with ciliary muscles, we can save ourselves much criticism by using the expressions "convergence muscles" and "accommodative muscles." For no one knows definitely anything whatever about the action of any one of the muscles of the eyeball, despite the dogmatic statements of our many speculative writers. No one knows anything definitely how or why these groups of muscles are brought into associated function. We may also point out that there has been a great deal of loose talk about the associations of ciliary muscles and internal recti in the positive act of accommodation and convergence. But who has said anything about the associations in the negative act, as when we change from near observation to distance observation? If there is association of muscles in the positive act, there must be associations of their antagonists in the negative act. If the abductors contract to pull the visual axes temporal-ward when we change from near to far observation and if some parts of the ciliary muscles act at the same time to flatten the crystalline lenses, then there must be in some form and at some place and by some method an integration of the control of those muscles in that act. All of the extrinsic muscles of the eyeballs are involved in some way, by reciprocal contractions and extensions and in synergic groups, and all the intrinsic muscles are likewise involved. But so long as we know so little about anatomy and functioning and synergic grouping of any of them it behooves us to speak more gently, less dogmatically, to use expressions more general, less specific, until we have the knowledge authorizing such specification.

We must look at the oculo-motor nuclei as an integrated group through which impulses are synergistically relayed for group action in the execution of functions. We may not safely select one, as the third or the fourth, or the sixth, as determining specific acts. But the determinations, selections and associations are brought about farther back in the functional tracts, at higher levels. We must also remember that there are a host of other influences, as tonicity, equilibration, and above all autonomic reflexes arising from the vegetative systems that are integrated and condition all ocular movements, just as they do all other muscular activities of the various organs of the body.

Convergence and Accommodation Association

Though there is little written on the subject, the physiologist and psychologist of today look at this convergence and accommodation act as an integrated, unit reflex, complex it is true, as are all functions. They look with wonder and amazement at our complicated theories of accommodative control over convergence and convergence control over accommodation. The term in general use is "The Visual Attention Reflex," and by that is meant the complex function of synergistically associated bi-foveal fixation and focusing. The laws governing attention and conditioned reflexes govern this.

The theories with which we have encumbered and stultified ourselves were formed in the days of philosophical psychology, when the facultative theories held sway, when to each function was ascribed an

hypothetical "brain center," acting more or less independently, and more or less controlling or subject to control by other facultative centers." All this has gone into the discard under the onslaught of modern experimental physiology and psychology. The tradition of plural mental faculties only survives in the writings of speculative ophthalmology. In attempting to explain the visual functions, the speculative writers of the nineteenth century seem to have seized upon the facultative theory and carried its concepts to absurd extremes. Thus we have such complicated theories, started from false assumptions, never based on actual experiment and investigation, that are proving something of a hindrance, at least of no help, in understanding and interpreting our problems.

Martineau's concise statement ought to clear the air for us. "In short, there is no facultative plurality in the mind: it is a single organ of true judgment for all purposes, congnitive and practical."

Psychologists, I find, listen with some surprise to a recital of such expressions as "accommodation incited by convergence," or "accommodation assisted by convergence," or "convergence brought into activity by accommodation," to such terms as "fusional reserves," and so on, and shake their heads in doubt, sometimes in amusement.

It is possible that we have fallen into trouble because of misconception of the idea of "function." When any group of muscles has come under associative control in any act necessary to our living or to our work or to our play, whether that associative control for that act is innate (developed during the months of gestation and in operation at birth or ready for functioning at birth, as breathing, suckling, crying), or is native (that is, habits or uses of muscles for the performance of some act that is normally expected of each member of the species, as in man, walking, speaking, binocular focusing, such acts being learned by personal experience after birth, environment being a large factor in their development, but always acts characteristic of and belonging to the particular species), or is a personally acquired and peculiarly individual act (as writing, playing ball, etc.), the activated association of those muscles for the performance of the act is a functional use of that group of muscles as used in that specified manner for that particular act, this act then becoming a "function" of that particular group of muscles. The nerve fibers and cells grouped, or integrated, by habit, for the associative control of the muscles are a part of the physical mechanism of that function. But that does not mean, by any inference, that the "function" is a mental attribute or a "mental faculty." Nor that any one "function" can control, modify or assist another. The function is the mechanism only, the physical part, the intermeshing of the parts for the delivery of the product. The function is not the power that operates the mechanism.

We may well rid ourselves of the habit of thinking that the functional uses of muscles in the act of accommodation can in any way affect the functional use of the muscles in producing bi-foveal fixation, or convergence. We may well set ourselves toward the elimination

from our technical phraseology of such terms as may indicate such ideas.

Sympathetic Innervation of Striped Muscle

Nearly all the points presented thus far have been negatives. But here is one extremely vital matter of positive character. That is the discovery that interspersed among the striated fibers of the voluntary external muscles of the eyes are smooth muscle fibers innervated by the sympathetic nervous system. Also, the peculiar fact that the behavior of the external voluntary eye muscles, in so far as this sympathetic system innervation is investigated, is different from any other muscles of the body.

In 1913, Ewing discovered smooth muscle fibers interspersed among the striped fibers of the superior obliques of the cat's eye, but did not pursue his investigations further, aside from demonstrating that their function was in some way connected with the maintenance of tonicity. A great deal of work has been done, dating from an earlier time that Ewing's studies, from 1904 to date, by a great number of experimental workers. There is a wide difference of opinion still as to whether all skeletal muscles are or are not supplied with sympathetic system termini and as to whether in every case there are interspersed involuntary muscle fibers or if in some cases the sympathetic innervation is directly into the striated fibers.

But the work of Kulchitsky and of Woolard, working independently, has shown positively a sympathetic system innervation of the external eye muscles in lower animals and in man, and that these connections are with interspersed smooth muscle fibers. There is still a question as to whether these are altogether from the spinal-sympathetic system or whether part are from the para-sympathetic. Para-sympathetics have been found in some other skeletal muscle groups.

But the eye muscles are more richly endowed with these sympathetic system collaterals than any of the other skeletal muscles so far studied. Also, as we have noted, the behavior of this eye muscle structure is different from that of other muscles.

Argument persists and will long persist as to whether this is wholly a provision for the insurance of muscular tonus or if there is some other functional requirement provided for that is as yet unknown.

However, there is sufficient data to give us an understanding of that phenomena of psychic origin that I have termed psychopathic esophoria. Though I recognized and analyzed this type of esophoria as distinct from the esophoria with latent hyperopia, which latter is the only type recognized in our text books, the operations by which it occurred were not understandable until the work of Kulchitsky was published in 1924 and the later confirmation that came with the publication of Woolard's investigations in 1927. (Kulchitsky: Journal of Anatomy, Vol. 58, page 152. Woolard: Comptes rendus de l'Association des Anatomistes, London, 1927.)

The effect of autonomic functions on human behavior, both mental and physical, both psychic and muscular, the effect of psychic,

or emotional, states on muscle tonus and muscle behavior has been amply demonstrated by the students of behavioristic psychology. The anatomical instruction that has been given us by the investigators into the sympathetic innervation of striated muscle has brought us something tangible that we can grasp in our studies of such problems as esophoria and exophoria, of esotropia and exotropia. That both are as much due to psychic states as to physical dysfunctions is quite evident. This anatomical evidence, along with some other points we have brought up in this paper, will eventually lead us to an understanding of squint, such as we have not previously had, particularly those types following such vegetative system disturbances as colic, typhoid fever and so on, or of those psychic types of squint following fright or anger.

There is a vast amount of literature in English, French and German on this subject, every word of which is worthy of careful study. Good digests of what has been done are presented in Woolard's "Recent Advances in Anatomy" and in Kuntz's "Autonomic Nervous System."

This is also applicable to a partial understanding of the behavior of the muscles in the duction tests. The quality of muscle chemistry is a large determining factor in the results obtained in the duction tests. This is modified by various factors, in which we must conclude autonomic and psychic disturbances and the reflexes therefrom.

The subject of the physiology of muscle exercise is undergoing continual modification as more and more facts are brought to light. This item of sympathetic innervations to striped muscle is one of the most important matters, in the study of interpretation of the behavior of muscle under exercise.

The autonomic nervous system has a profound effect on the mental behavior of persons and the reverse is true, the influence of mental reactions has a far reaching effect on the functioning of the autonomic system. When we find that by anatomical connections these reflexes are carried into the voluntary muscles and affect their chemistry and behavior, we are somewhat awed at the task before us of learning what the duction tests really mean. We must blush at the thought of the speculative theories so argumentatively set forth in the past.

Conclusion

In the opening page of "A Manual of Ocular Neurology" by de Lapersonne and Cantonnet (Paris, 1923), we read that the study comprises three parts: A, the apparatus of execution, the muscles; B, the apparatus of transmission, the nerves; C, the apparatus of command, the "ensemble" of the intra-cephalic centers.

And therein lies the whole story. In the words of one of our speakers at the meeting of this Academy in Chicago, "When you place a lens before an eye you are practicing neurology."

Optometry's practical problems center in the behavior of the central nervous system, its reactions to the artificial condition created by placing lenses before the eyes. It is not what happens to a beam of

light when it passes through a lens that concerns the optometrist, but what happens when that beam of light strikes the retina and starts a long chain of psychic reflexes.

The problems of binocular balances and imbalances are not problems in geometric and physical optics, but problems of the behavior of the central nervous system.

For example: In the duction tests, we displace an image by means of a prism. The sensation is conducted to the central nervous system provided the sensory nerve is conductive. Some neurons in the cortex finally receive the message of the off-foveal image which creates an electro-chemico disturbance, perhaps. If it does, if the annoyance is sufficiently irritating, and here the psychic element enters, a motor reflex is started on its way down a motor nerve. If the motor nerve is conductive, the impulse reaches the muscle. If the muscle is in chemical equilibrium it contracts. And here enters another problem, inhibition must reach the antagonistic muscle which must be in such chemical state that it can extend. If all these factors and several others that we will not pause to consider are favorable, the fovea turns to the displaced image and we say that duction is "good." If these physiological and psychological factors are not favorable, the fovea does not turn and we say the duction is "poor."

Hence, our studies may well be directed toward an understanding of these three subjects, in which the whole of psychology and physiology are involved: The apparatus of command, the apparatus of transmission, the apparatus of execution.

There is an immense storehouse of physiological and psychological data at our disposal in the newer books and current periodicals. In the time appropriate to the presentation of this paper, we cannot begin to speak of the many recent discoveries of vital importance to the successful practice of refraction.

The answers to our problems are being written by the experimental psychologists and neurologists. The very fact that experimental psychology devotes a very large proportion of its efforts to the study of visual functions ought to show us that there is the place to look for help, rather than in the moribund speculations of dead and gone ophthalmologists.

I have purposely selected items of a negative type for this paper in the hope of showing how fanciful are the theories we have fostered. Before we can begin the erection of a more substantial foundation for the art and practice of refraction, we must realize the weaknesses of the old. The new must be built from the solid stones of accumulated facts, with a great effort to gather as many facts as we can, bound together with the strong mortar of empiric research, with constant caution against its dilution by hasty conclusions or theoretic interpretations.

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THE HOROPTER*

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The subject of the horopter has always been a source of disagreement among physiologists and its conception varies according to the author. The writer fully realizes his limitations and will not attempt a new conception, but rather an elucidation, as the subject is important for the complete understanding of binocular functions.

Such classical description as Helmoltz's, that the horopter is "a line of double curvature" or "the intersection of surfaces of the second degree hyperboloid, and the surface of a cone or cylinder" is intriguing, but means nothing to the reader and only serves to scare him away from the subject. It is possible, however, to present a practical and understandable definition, which, while not mathematically precise, still concords with general facts. It is necessary before proceeding further to refresh in the mind of the reader, on the subject of corresponding retinal points.

Muller stated that were the retinas of both eyes laid one over the other with their principle meridians coincident, all points in contact above and below would be corresponding points. Hering, Helmoltz, Volkman, Donders and others elaborated and stated that while this theory applied to points lying on the horizontal meridians, it would not apply to points lying on the vertical meridians which were found to diverge 1¼° each at the top. This divergence they assumed to be physiological. Stevens stated that not only did the vertical meridians tilt, but all meridians rotated around a saggital axis (antero-posterior) for different positions of regard. This proposition is undoubtedly true and this rotation must affect the horopter, but it is not necessary to postulate on it in consideration of corresponding retinal points as will be seen later.

There is no reason to believe that a minute retinal point coincides in both eyes with a minute cranial center point. If not, it is reasonable to assume that no certain cranial center point is identified with a specific direction in space, but rather, that the brain has capacity of selection and

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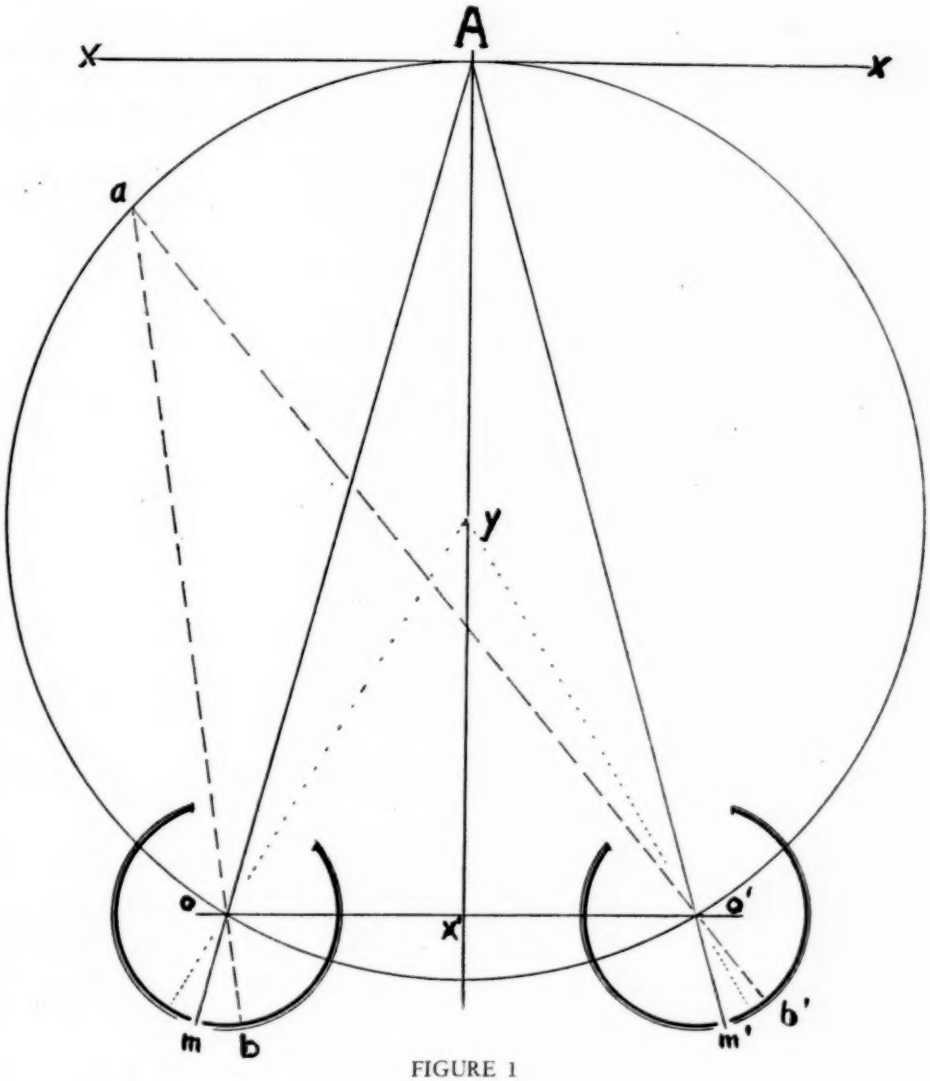

FIGURE 1

judgment of location is made by estimation of angular values. If the brain were a telephone switchboard, not so many would see singly in spite of their high phoria or tropia. We know that in squint cases the macula of the deviating eye is not the center or reference point of that eye. As a rule a new center has been developed by the brain, yet no anatomical change has taken place, the brain merely transfers the seat

of localization from one area to the other, exactly as it did in infancy when it acquired the faculty of localization.

When an object is fixed binocularly, the retinal image of that point of fixation falls in each eye exactly on the macula, secondary images fall on other points of the retina, those on the right side of the object fixed, falling on the left side of the macula, vice versa and etc. We shall now demonstrate that only a limited number of retinal points are corresponding, the greater number being disparage points, thus producing a diplopia of which we are not conscious.

In Fig. 1, the two eyes O and O' are fixing binocularly the point A. Its image falls at the maculas m and m', these being corresponding points. If we now consider a as another object seen indirectly in the field of view, its image falls at points b and b' of the retina of each eye. The angles subtended by the visual lines $aO'A$ and aOA are equal, therefore the arcs mb and $m'b'$ are also equal, b and b' are equally removed from the macula and are therefore corresponding points. *The object a is seen singly.*

If the secondary visual point a was removed to some place along the plane xAx other than A its image would not be seen singly because the angles subtended by the secondary lines of sight with the lines of fixation would not be equal, the arcs subtended on the retinas would also not be equal and the images b and b' would fall at unequal distances from the maculas m and m' thus being seen double.

We can state, therefore, that any object situated along the arc $O\,a\,A\,O'$ (limited of course by the binocular field of vision) will be seen singly and that its image will fall on corresponding points of the retina, and also that any object situated beyond or nearer than that curved plane falls on non-corresponding points and is seen double.

For instance, the image of an object situated at y will be received by the right eye at a point between $m'b'$, while in the left eye it will fall at a point to the left of m. These two points will be equidistant from the macula but on opposite sides, and if y were in a higher plane than A the diplopia would be still more pronounced, due to declination as can be demonstrated by holding a pencil in the median plane with fixation at infinity. We may now describe corresponding retina points as *those points in the retina which for a given point of fixation are equally removed in a linear direction from the macula and which if projected back into space meet at a point somewhere on a circle passing through the point of fixation (A) and the centers of rotation (OO') of the two eyes.* While this statement is not precise, it is accurate enough for a clear conception of the subject.

Why is the brain not conscious of the diplopia produced by all other points in space not situated on that circle? We may here quote Stevens: "Are the images of these points or objects which are seen doubly mentally suppressed? Does the mind refuse to shut them out, refusing to take cognizance of them, or do they actually constitute an important if

not an essential, part of the physical impressions which unite to constitute the basis for a complete mental conception of the field of view?" Undoubtedly the latter contingency is the correct one.

This diplopia serves the brain in interpreting the ocular image, probably permits it to estimate distance and position. The process by which it takes place is unknown and solely speculative.

It is apparent from the above description that corresponding points on the retinas are so only for a given fixation and that when the fixation changes these same two points become non-corresponding points. Yet as long as fixation is on any part of the circle OaAO′ corresponding points remain so and any object on this circle is seen single.

It is remembered that any three points in space can be enclosed on the circumference of a circle. Therefore, every point of fixation is one of three points in an imaginary circle, the other two being the centers of rotation of the eyes. The further away point *A* is from the base line *O′O* the larger this circle is until when *A* reaches infinity the arc *OO′* becomes almost flat.

This circle is the *horopter circle* for that particular spot of fixation *A*. Each point of fixation has a horopter circle of its own, comprising the point of fixation and the centers of rotation of both eyes. *The horopter circle may be defined as that line passing through the point of fixation and the centers of rotation of the eyes, and along which any object situated will appear single.*

When we say each point of fixation has a horopter circle of its own, it is meant for each point along the median plane *x′A* as any point outside of the *x′A* plane would merely duplicate a horopter circle of some point along *x′A*. For instance, with fixation at *a*, secondary visual image *A* falls on corresponding points of the retina and point *A* itself coincides with the horopter circle *OaAO′*.

Let us now assume that *A* represents the nearest point of binocular single vision and let us overlook segment *OO′* entirely. Now with the head erect and the eyes in the primary position, if we rotate a segment of the circle *OaAO′* along chord *OxO′* up above and down below the horizontal plane as far as binocular fixation and single vision permits, we produce a surface like a large segment of a hollow sphere, this segment of an irregularly curved outline or edge.

This surface is the horopter surface for that particular plane of fixation. Obviously the path described by point *A* in this rotation is a curve in the vertical plane *Ax′*. Now we recede point *A* from the nearest point of binocular fixation to infinity, we may properly call the space inclosed between these two imaginary surfaces: the *horopter*. At this point the horopter dangerously verges metaphysical and we had best leave it alone and return to the horopter circle. Let us remember, however, that the plane of these horopter or isogonals circles from the nearest to the farthest, are movable planes that can be rotated along axis *OxO′*.

The horizontal horopter circle can be determined mathematically or subjectively, by means beyond the scope of this paper. We have treated of the horopter as a curve coinciding with the segment of a circle. This statement is not exact and is made for simplicity only. The determination of the horopter curve is dependent on the optical properties of the eyes and the nature of distribution of retinal elements. The horopter curve generally assumes the form of an ellipse and in squint cases there may be no horopter or at least a false horopter due to non-macular fixation as mentioned above. Of what use is the horopter? It is necessary for a complete understanding of ocular images, of binocularvision and orientation, the same as an understanding of the dioptric system is necessary for the proper use of lenses.

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THE USE OF CALOBAR RED-FREE ABSORPTION GLASS IN CERTAIN CATARACT CASES

The subject of using absorption glass in certain selected cases of senile cataract with negative histories, has been given attention by optometrists, from time to time for the last thirty years. No organized attempt, however, had been made to compile the results of the use of absorption glass, in these cases, until an Academy Committee, under the direction of Dr. Elmer E. Hotaling of New York City, started work on this subject about five years ago.

Hotaling[1], after some experimental work in this field upon fish, and after the presentation of papers on this subject by the writer[2] and by Melvin[3], started compiling the results of the use of red-free[4] glass in a large number of these cataract cases. His preliminary report of the committee of which he is chairman, was presented to the profession in the December issue of this Journal[5]. This initial report is very complete. It

[1]Hotaling. Elmer E.

[2]Koch, C. C. Invisible Radiant Energy as the Cause of Cataract. Am. J. Optom. Vol. 7, No. 6, pp. 325-336.

[3]Melvin, O. J. Calobar Glass Therapy. Am. J. Optom. Vol. 7, No. 6, pp. 337-339.

[4]Calobar Glass as made by the American Optical Co., Southbridge, Mass.

[5]Hotaling, Elmer E. Report—Red-Free Glass in Certain Cataract Cases. Am. J. Optom. Vol. 9, No. 12, pp. 434-442.

deals with 193 individual eyes, each suffering from senile cataract, and in each case, care was taken to ascertain that the patient was free from both systemic and organic involvements before these lenses were prescribed.

In all of these cases, Calobar corrections were prescribed. In most, the medium shade was used, and of the 193 cases reported upon, all but 41 were refracted at subsequent intervals, so as to enable Hotaling to observe the results of this experimental technic. Deducting these 41 unobserved cases from the total number of 193 eyes treated with this therapy, leaves 152 eyes which were kept under observation from periods varying from one and one-half to five years. Of these 152 eyes, 75 eyes showed an improvement in visual acuity after using a calobar red-free correction. In 41 eyes the resultant visual acuity, after the use of the treatment correction was the same as at the start, while in but 36 eyes was the visual acuity found to be reduced, and in none of these cases was an operation necessary.

Hotaling's report, of course, gives much other material of interest in this piece of clinical research. However, the fact that approximately fifty per cent of the cataract cases reported on, showed improvement in visual acuity, following the use of this calobar red-free therapy, indicates, from the standpoint of clinical results, that the theories, upon which this work was based, have in a measure, been substantiated.

C. C. K.

* * * * *

THE UNRELIABILITY OF HURRIEDLY TAKEN PHORIA FINDINGS

Phoria tests are at times hurriedly made and this haste frequently leads to inexact findings and puzzling results. It is well to remember in making a phoria test that we are in reality measuring the amount of neuro-impulses received by those of the extra-ocular muscles involved in the test. Now, as is well known, the amount of nerve force reaching these extra-ocular muscles is constant in quantity and quality unless stimulated by some outside force or by some accommodative impulse. When this stimulation occurs the total amount of the neuro-innervational impulse may vary considerably. For instance, placing a dissociating prism before one eye, or a maddox rod and a colored disc before the two eyes may cause a temporary stimulus which may take as much as a minute or two to subside. The findings, if made before the neuro-impulses are again stabilized may be totally at error and therefore lead the examiner far afield. A case of this character was anonymously presented some four years ago.

> "A woman came back to me the other day. One of her eyes, the eyelids, and even her face were swollen, clear down to beneath her jaw. She complained that the glasses I had made for her were the cause. Believe me, I could imagine her going somewhere else and also telling the woman who referred her to me and

all their acquaintances. In making the first examination of her eyes, I had found no muscle imbalance to speak about and, on checking up her correction I found the same thing, but in testing her vertical balance with the Maddox rod and light I found that, after looking at the two images for a few moments, the streak started to wander away up. I evidently did not take enough time for this part of the test in the first examination, but believe me, I will always take sufficient time in the future to see what transpires within a few minutes' time.

"I gave the woman the same correction with the addition of a prism base up in one eye, and as she has worn them since with perfect comfort, and all the other symptoms disappeared. I take it for granted that the hyperphoria was responsible for the same."[1]

This case illustrates the difficulty which may come to the examiner through the careless or hurried taking of the phoria findings. While care and attention are required in making all optometric refractive tests, it is, of particular importance not to hurry unduly, the taking of the phorias. It is important that sufficient time elapse, after the test is started so as to enable the ocular accommodative-convergence neuro-innervational impulse to again become normal and constant. C. C. K.

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THE USE OF INFRA-RED RADIATIONS IN ORTHOPTIC WORK

In the Post-Graduate Eye Institute Associate Monthly Report for December, 1932, Shepard and Mendelsohn suggest the use of infra-red radiations as a pre-orthoptic treatment technic, claiming that additional innervation can be more rapidly developed through the proper application of heat.

They say in part: "Our observations have not led us to the conclusion that corrections can be accomplished through the use of applied heat that would be impossible without it, but we are of the opinion that corrections are made somewhat more rapidly and perhaps more easily when the infra-red lamp is focused upon, or in the region of the patient's eyes during the first five or ten minutes of the exercise period."

This, it is interesting to note, is very much the same conclusion that both the writer and Kurtz arrived at several years ago when we first suggested the use of high-frequency and diathermy in these cases of ocular neuro-muscular involvements. That the results are similar is not surprising as these currents are found in the intra-red end of the spectrum. While we have had no experience in using an infra-red lamp in these cases we have had considerable success with the application of heat applied to the eyes through the use of electro-therapeutical agents and can recommend that additional study be made of all of this important optometric work. C. C. K.

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ANGSTROM UNITS

A reader requests an explanation of the term Angstrom units. He understands that it is a term used to measure various forms of radiation, but he does not understand just how the term came into being.

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[1]Optical Journal, September 7, 1928, p. 22.

In the first place, an Angstrom unit is an arbitrary unit of measure that has been universally adopted by scientists and others interested in radiations of all types, to measure the wave length of both the visible and invisible radiations. Just as the term mile has been adopted to measure distance or the term pound has been adopted to measure weight, so has the term Angstrom unit been adopted to signify a unit in the measure of all forms of radiation. These and other terms have been arbitrarily set aside and accepted by all users as a measure of standard by which similar things can be compared.

The term Angstrom unit was named after Anders Jonas Angstrom (1814-1874). The unit itself is one ten millionth of a millimeter in length. In inches we would find this to be two hundred and fifty-four millionth of an inch. To better appreciate what an Angstrom unit really is, let me say:

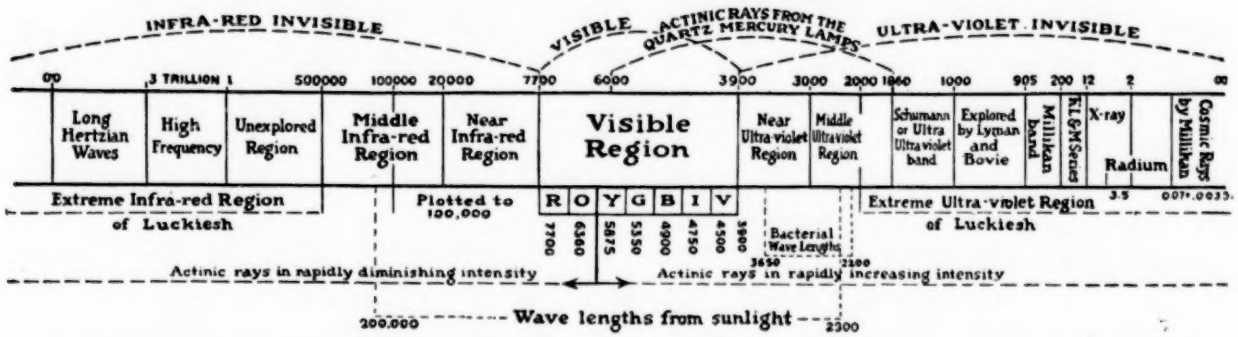
One meter equals one thousand millimeters.
One millimeter equals one thousand microns.
One micron equals one thousand millimicrons,
and, One millimicron equals ten Angstrom units.

Now as scientists studied, first the visible spectrum and later on the invisible spectrum, they found that various portions of this spectrum had very dissimilar properties and in experimental work, found, that in separating the spectrum into various parts that the effects of one portion upon matter was different from that of another portion. They also found that in both the visible and invisible radiations the energy or force with which they were dealing came in waves and to differentiate one group from another they measured these waves from crest to crest, and as in each particular band they found these waves to be always constant in length they termed the particular band measured from the crest of one wave to the crest of the next, by the numerical number of ten millionths of a millimeter that these scientists happened to find it. For instance, radiations 7,000 ten millionth of a millimeter in length (crest to crest) have the property of producing the sensation of visible red when absorbed by the retina of an eye. However, it is difficult to say, "7,000 ten millionth of a millimeter," so the "ten millionth of a millimeter," part was dropped and the term Angstrom unit was substituted. In writing, this term is usually further abbreviated to A.U. or A.*u*. Thus for instance, in referring to those radiations having the property of stimulating a normal eye in such a way as to cause the sensation of red, we merely write the formula, 7,000 A.U.

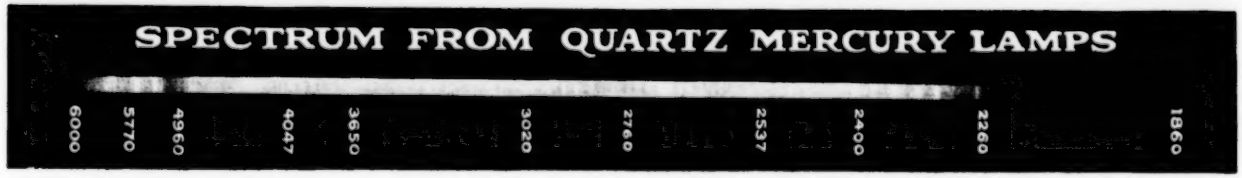
Of course, similar radiation of slightly different length also will produce the visual sensation of red. In fact, all radiations from 6,200 A.U. to 7,700 A.U. produce the sensation of red, each in a different degree or shade. Those nearer 6,200 A.U. produce the orange-red while those nearer 7,700 A.U. produce the darker and deeper shades of red.

I have mentioned both the visible and invisible types of radiation. There is absolutely no difference between the two except in the length

DIAGRAMMATIC SPECTRUM ANALYSIS



SCALE IN (Å) ANGSTRÖM UNITS



of the radiation or waves of the two groups. The reason for referring to one of these groups as visible and to the other as invisible is that those radiations whose wave length is between 3,900 A.U. and 7,700 A.U. have the property of stimulating the normal retina in such manner as to produce the sensation known as vision. Radiations varying in length from 0.0001 A.U. to 3,900 A.U. are in theory at least, essentially the same as the visible yet they do not reach our retina, being entirely absorbed by the air or the various media of the eye, as in the case of the ultra-violet or else they pass entirely through the tunics of the eye as in the case of the X-rays and other of the short invisible radiations, and therefore produce no visual stimulation. Those radiations whose wave length varies from 7,700 A.U. on up to 5 trillion A.U. and beyond are also essentially the same as the visible radiations between 3,900 A.U. and 7,700 A.U. yet these are also invisible as the retina cannot transfer the energy of these waves into visual impulses. Therefore, we find that radiations whose wave length varies from 0.0001 A.U. to 3,900 A.U. are invisible. Those from 3,900 A.U. to 7,700 A.U. so effect the retina as to produce visual sensations and those from 7,700 A.U. to the limit around 5 trillion A.U. are again invisible. This first group (0.0001 A.U. to 3,900 A.U.) are comprised of the cosmic rays, the gamma or radium rays, the X-rays and the extreme and vital ultra-violet rays. As illustrated in Dr. Plank's accompanying chart, these are all invisible. The small visible group (3,900 A.U. to 7,700 A.U.) are composed of the visible spectrum, violet through to red, and the third group (7,700 A.U. on up to the limit of the long rays) are composed of the short and long infra-red rays, high-frequency currents and electro-magnetic radiations of various types, all of which are also invisible to the eye. The following list gives a few of the principal classifications and the general limits of their wave lengths.

Spectrum Analysis

Cosmic rays	0.0001	to	0.10	A.U.
Radium or gamma rays	0.10	to	3.	A.U.
X-rays	3.	to	12.	A.U.
Millikan band	200.	to	900.	A.U.
Extreme ultra-violet	900.	to	2,000.	A.U.
Extra-vital-ultra-violet	2,000.	to	2,900.	A.U.
Vital-ultra-violet	2,900.	to	3,200.	A.U.
Intra-vital-ultra-violet	3,200.	to	3,900.	A.U.
(Visible rays.)				
Violet	3,900.	to	4,300.	A.U.
Blue	4,300.	to	4,700.	A.U.
Blue-green	4,700.	to	5,000.	A.U.
Green	5,000.	to	5,300.	A.U.
Yellow-green	5,300.	to	5,600.	A.U.
Yellow	5,600.	to	5,900.	A.U.
Orange	5,900.	to	6,200.	A.U.

Red	6,200.	to	7,700.	A.U.
(This being the limit of the visible radiations.)				
Short infra-red rays	7,700.	to	20,000.	A.U.
Long infra-red rays	20,000.	to	500,000.	A.U.
High frequency waves	1 trillion	to	3 trillion	A.U.

This in a brief manner outlines a few of the generally accepted theories of the spectrum. A large amount of research work has been done on the study of various portions and much more is still to be done. It is obvious, however, that in a study of this sort, some unit of measure had to be devised and the term Angstrom unit was the outgrowth of this need.

It is also interesting to note that the visible radiations with which we as optometrists deal, are in a sense essentially the same type of radiations as are those exceedingly short and little known Cosmic radiations now so much in the public interest due to the recent balloon experiments in Switzerland. Our visible radiations are also in a sense similar to the short X-rays, the ultra-violet rays, the longer infra-red rays, all types of electrical currents and also the various forms of undampened radio electro-magnetic radiations. These last and extremely long radiations are, to be sure, not at all suitable for the formation of visual impulses but nevertheless have many of the same characteristics.

C. C. K.

BOOK NOTICES

THE PRINCIPLES OF OPTICS, Arthur C. Hardy and Fred H. Perrin. Published by the McGraw-Hill Book Company, Inc., 330 West 42nd Street, New York, N. Y. 632 pages, 319 illustrations. Cloth, $6.00, 1932.

In this book the authors attempted to present The Principles of Optics in a way that will not require the use of complicated mathematical treatment and they have to some extent succeeded. It is intended as a text for students in optics as well as for those who specialize in pure optics and applied optics.

The first five chapters deal with geometrical theory of image formation. This part must necessarily be treated somewhat from a mathematical standpoint. Chapter seven deals with diffraction and the limitation it imposes upon optical systems. The next seven chapters treat the properties of radiation and light sources, the detectors of radiation such as the eye, the photoelectric cell and the measurements of color and light. The remaining chapters describe the properties of optical material, the construction of optical instruments, the testing of parts and the principles underlying the design of optical instruments.

This book is the result of actual experience in teaching the principles

of optics at the Massachusetts Institute of Technology and is a valuable text for students in optics as well as for the optical engineer.

J. I. K.

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INTRODUCTION TO APPLIED OPTICS, Volume 2, L. C. Martin. Published by Isaac Pitman and Sons, 2 West 45th Street, New York, N. Y. 289 pages, 203 illustrations. Cloth, $6.00, 1932.

This book was written by the author as a result of the great reception given to volume 1 of Introduction to Applied Optics. It was the aim of the author to present in this volume 2 only subjects not adequately covered in other books on this subject.

In the first chapter the author describes the principles of magnification produced by lenses and lens systems. In the next two chapters the telescope and microscope are described. Chapter four is of particular interest to the refractionist, as here the physiology of binocular vision and binocular instruments are fully described. A unique diagram is presented illustrating the visual tracts and the connections of the third, fourth and sixth nerves. The workings of the stereoscope is thoroughly explained accompanied by a number of diagrams illustrating the construction of the different types of stereoscopes. The next three chapters describe fully photographic lenses, the photometry of optical system and the testing of optical instruments.

For the student of optics this book offers much valuable information.

J. I. K.

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ARTIFICIAL LIGHT AND ITS APPLICATION IN THE HOME. Prepared by the Illuminating Engineering Society. M. Dygert, Chairman. Published by the McGraw-Hill Book Company, Inc., 330 West 42nd St., New York, N. Y. First Edition, 145 pages, 43 illustrations. Cloth. 1932. Price $1.50.

This book, Artificial Light and its Application in the Home, was prepared by the Illuminating Engineering Society, committee on residence lighting. It was the aim of the committee to present a practical and reliable treatment of artificial light in the home.

A considerable amount of information of value to the refractionist is to be found in this little book. Among the many topics discussed are to be found: Light and Vision; Radiation and Health; Control of Brightness; Reflection Factors of Colored Surfaces; Rainbow Composition of Light; Color in Light; Psychological Significance of Color and its Effect; Colored Light and its Effect on Colored Subjects.

The system of direct and indirect lighting is fully explained; also the elimination of glare and shadow and how this affects the comfort of the eyes and nerves.

J. I. K.

ORTHOPTIC TRAINING WITH THE STEREO-ORTHOPTOR. Published by the American Optical Co., Southbridge, Mass. 74 pp. Illustrated. Cloth. 1933.

This monograph is a handbook for the new treatment instrument introduced to the profession this month by the American Optical Co. The various uses of the Stereo-Orthoptor as an aid in treating squint, amblyopia ex anopsia and defects of the fusion sense are reviewed and the instrument itself clearly described.

The Stereo-Orthoptor is an instrument made to simplify the procedure of orthoptic training and at the same time to intensify the effect of the treatment. It is believed that the automatic simplicity of its manipulation will prove a time saver to the optometrist. Various technics are suggested.

The book is conservatively written. The professional optometrist specializing in orthoptic treatment work will find that the authors had him in mind in writing the text and will find practical suggestions which should be of value in this important field of optometric work. It is gratifying to note the trend, on the part of the large manufacturers, to recognize the importance of the professional man in optometric work and also to note that these executives are becoming more and more interested in the professional phases of optometric procedure and to find them ever willing to co-operate with those practitioners who are practicing in an ethical and professional manner.

The entire book is written from a professional viewpoint and the American Optical Co. is to be congratulated in placing this stimulating force for ethical practice before the profession, for both the instrument and the book describing it are aids in building a professional practice. The first chapter dealing with Orthoptic Treatment Rooms should be read aloud before every local optometric society in America.

C. C. K.

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THE BOOK OF VISION. Published by the Bausch & Lomb Optical Co., Rochester, N. Y.

This book has its greatest merit in its appeal to the average patients who require the attention of optometrists. It is leather bound, beautifully illustrated and graphically conveys the message intended, i.e., that the proper examination and fitting of spectacles is a necessity in this modern world. The pictures on pages 10 and 11 would appeal especially to those parents who are reluctant about having their children wear glasses. These pages also would have a good psychological effect upon the child who suffers from an inferiority complex caused by the wearing of glasses. The illustrations on pages 24 and 25 convey to the average person the fact that the evils of glare cannot be escaped but that the harm done by it can be overcome by the proper attention given to the eyes. This book should be valuable when placed in the reception rooms of optometrists.

J. I. K.

ANNOTATIONS
AND
SOCIETY PROCEEDINGS

The American Journal of Optometry is the Official Organ of the American Academy of Optometry, as well as the Organ of the Arkansas, Delaware, Indiana, Iowa, Maine, Maryland, Michigan, Minnesota, Nebraska, North Dakota, Oklahoma, Pennsylvania, South Dakota, Vermont and West Virginia Optometric Associations.

The following gentlemen have consented to supply items of interest from their respective sections, acting in the capacity of

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OPTOMETRIC EDUCATIONAL SCHOLARSHIPS

Dr. C. W. Morris, optometrist of Fort Wayne, Indiana, writing on matters of optometric education, says in part: If optometry should adopt the educational program advocated in a previous article[1], some will point out the fact that for a period of about ten years optometrical education might suffer a temporary restriction in the number of students due to the fact that professional optometry cannot be sold to the public in a short space of time. I would like to suggest a possible way to meet that situation which would be effective in selling optometry to the public as well as in providing an adequate supply of the highest type of students, teachers and research workers for our optometry schools.

Competitve scholarships in optometry is the recommendation in brief. This proposition could be financed by adding ten dollars to the present renewal fee of each registered optometrist. If the optometrists in California can divert eight dollars of their renewal fee to endow a course in Optometry at the University of California (Optometry's Educational Problem by Dr. Peckham in the American Journal of Optometry for

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[1]C. W. Morris. Optometrical Education. Vol. 9. No. 11. American Journal of Optometry. 1932.

July, 1932), it ought to be possible for the board to do this, particularly when the board is empowered to determine annually the amount of the renewal fee within prescribed limits as it is in three states at the present time. These scholarships would be competitive and advertised in all the high schools. They might be worth as much as four thousand dollars and cover a period of four years.

If the optometry schools were unable immediately to offer the necessary work in anatomy, histology, pathology, physiology, embryology, pharmocology and bacteriology, arrangements might be made with some medical, osteopathic or homeopathic medical college to provide facilities for the first two years' work. The last two years' work would be given in the optometrical colleges and, with the exception of courses in local eye pathology and lectures and clinics in general pathology as related to the eyes and a general knowledge of ear, nose and throat work as related to eye symptoms, the major part of the last two years' work would consist of optometrical subjects proper with only a very slight emphasis upon physics and theoretical optics.

This plan, if adopted by all states, would probably put about four thousand men of the highest qualifications into the practice of optometry within a period of ten years. I believe we have something like twenty thousand men in practice. At ten dollars per year two thousand men would be kept in school on the basis of one thousand dollars per year for four consecutive years. After ten years, if not before, the impetus given optometry would be so great that the problem would be how to restrict the number of men seeking to practice optometry and to provide better instruction and physical equipment for the schools.

This plan also takes care of a most vital need for an expanding optometry, how to provide teachers with degrees in optometry and with doctor of philosophy degrees in various other sciences in addition to the strictly optometrical subjects. Such a class of educators would make us independent of, and equal to medicine in our facilities to give instruction in the first two years of medical work.

A few men would be retained each year by appointments to assistantships in the clinical laboratories with the provision that they would become candidates for the degree of doctor of philosophy in some subject which would make their services of value to optometry as teachers. We would then have a splendid class of men, second to none, teaching every subject in the curriculum of the proposed expanded course of optometry of the future.

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REFRACTIONISTS IN ENGLAND

The subject of a name for nonmedical refractionists in England has been occupying a considerable portion of the optometric professional press of that country. The editorial board of the *Optician* is strongly in favor of this group using the old term "optician" instead of the more modern one, "optometrist." The

latter name has been suggested in several quarters, by English refractionists.

While they admit that some 25,000 other English speaking non-medical refractionists have adopted the term optometrist, they nevertheless believe that American, Canadian and Australian optometrists are wrong and that the few thousand refractionists they speak for are right. It looks very much like a case of, "—they are all out of step but Jim."

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MASSACHUSETTS BOARD OF REGISTRATION

The Massachusetts Board of Registration in Optometry will hold its next examination on June 12, 13, and 14, 1933, at the State House, Boston, Mass. Applications for examination should be on file at the office of the Secretary, Room 141, State House, at least ten days before date of examination.

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USE OF THE KERATOMETER

A new folder by Haller Belt, entitled, "Observations on the Use of the Keratometer," is now being distributed by the Bausch & Lomb Optical Co. The folder describes not only the scope of the instrument but explains as well just how findings are obtained with its use. A copy of the folder will be sent without charge on request to the Instrument Department, Bausch & Lomb Optical Co.

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CLINICS IN SOUTH DAKOTA

The Skeffington Post-Graduate Clinics were held in South Dakota the week of January 22. The first three days, 22-23-24, Skeffington was in Sioux Falls, S. D., at the Cataract Hotel. And the 26-27-28 in Aberdeen, S. D., at the Ward Hotel. The clinic in Aberdeen was a huge success. There were approximately fifteen in attendance. The meeting here in Aberdeen included the Central District, the Northeastern District, and the North-Central District.

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SOUTHEASTERN CONGRESS OF OPTOMETRY

The Ninth Annual Southeastern Congress will hold its regular meeting at St. Petersburg, Fla., May 28-29-30-31, at the Suwannee Hotel. This is the annual educational meeting of North and South Carolina, Virginia, West Virginia, Mississippi, Tennessee, Kentucky, Alabama, Louisiana, Georgia and Florida.

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BACK COPIES WANTED

Dr. Stanley S. Susskind, of 7 South Fourth Street, Mt. Vernon, N. Y., is anxious to secure a March, 1932, issue of the American Journal of Optometry. Any one who can spare a copy is requested to get in touch with Dr. Susskind.